In this work, the authors perform a widespread tracing of a ~4.72ka layer across significant region of West Antarctica. This is a valuable data set and the authors robustly show how it can be used with simple models to infer the average accumulation conditions around this time. The paper is clearly written and the outcomes clearly presented. I enjoyed reading this contribution and I have mostly minor comments to consider.

One potentially more major consideration, however, is that the lead statement in the abstract, "Modelling the past and future evolution of the West Antarctic Ice Sheet (WAIS) to atmospheric and ocean forcing is challenged by the availability and qulaity of observed paleo-boundary conditions", sets up this work for it's value providing a new constraint on models. I agree that this is an important contribution, but more specific statements about how this new layer product and new inferences of past accumulation can be (or could be) used in models would be helpful. For example, it isn't clear to me if these constraints are valuable to regional ice-sheet models, continent-scale ice-sheet models, and/or also climate models. How could these outputs and inferences practically be used in models, and how many of the current generation of models are set up to use constraints like this in the way they have been shared? For example, few ice models assimilate layers, but is that the emphasis here – that even on Holocene timescales that layers are an important constraint on models, especially in the ice-sheet interior? Or, is it more to advocate using simple models to infer accumulation histories that can then more directly be used as boundary conditions in a wider suite of models? (Or, both!) If there are more specifics that can be added then I think it could have more impact on the modeling community. Some points are mentioned in the manuscript, but structuring parts of the manuscript around this more specifically would be worthwhile if it remains a main motivation and a main conclusion.

Line 25 (related to above point): If possible, I would suggest trying to clarify this point so it is more directly speaking to the modeling community. How do these results advance what is required for model spin up, and if these are continent-scale models then why is this region of West Antarctica and over this time so critical to improve model spin up? Minor rephrasing and a few more words could help make this a more impactful point

Line 38: Since mention comparison to modern in this sentence, could indicate time range over which 18% increase occurs during the mid-Holocene – would help clarify why increase of this amount is important (compared to seasonal or inter-annual variability, for example)

Line 42-44: In general I feel like this point could be a bit more developed, especially if these results are meant to motivate ice-sheet modelers to use new records like this. There may not be space, but a few more words on why modeling past sea-level rise is important could be worthwhile. Elsewhere present and future sea level are also mentioned.

Line 54: Sentence is about modeling changes in ice volume and GL position, so wonder if some more recent references are worth including

Line 56: This is a personal reaction, but the paper has a number of acronyms so it could be worth considering to keep only those that are necessary. I think "GL" could just be given as "grounding line", but again that may just be personal preference and I leave it to the authors to decide! [As an example, it became challenging to read in the text around Figure 1 – as many of these acronyms come into the text – but not sure the best way to handle that.]

Also, could specify what grounding line you are referring to here

Line 71: Could cut "At present"? Perhaps expand this point a bit to clarify if you mean climate models, ice-flow models, or both?

Line 76: By "model physics" do you mean our physical understanding and how that can be represented in models? That could be through parameterization, but guessing that you are also indicating that some physics of the system are still unknown. Could be a bit more clear.

Line 82 and 83: In this list of paleo records, are all of these available to inform WAIS? This paragraph could indicate what motivates the problem generally vs. specifically for West Antarctica. It would help to put in the context of whether the models are regional or continent-scale.

Line 85: Could explain a bit more why radar is an "alternative data source"

Line 92: It isn't completely clear what is meant by "suitable datasets" – entirely an issue with the radar data?

Line 144: Since this is where the data sets are being introduced, could consider stating age range that they cover even more explicitly

Line 184-: Perhaps it is worth mentioning that these layers are well dated, which in this calculation of the relative uncertainty is what leads it to being a low estimate? Or, consider to bring the point up front in the paragraph about this not accounting for uncertainties in the model approximation. 3.3% uncertainty seems low so it could help the reader to explain that more and perhaps define more clearly what the relative uncertainty is

Line 194: I think that MacGregor et al. 2016 only used the LLA (but it would be worth double checking)

Line 200-: "…but because the value of D…" –I think that I understand the point being made, but I think it could be confusing so suggest revisiting

Line 205: This sentence could indicate that a vertical strain correction is still applied based on the model you apply ("sole result of accumulation rates at the surface" doesn't capture that)

Not sure if/where it belongs, but could also considering citing Leysinger Vieli et al. (2011), JGR Earth Surface 10.1029/2010JF001785 for their evaluation of the applicability of vertical flow models to infer accumulation rates from East Antarctic layers (though to find that a 3-D model was necessary)

Could also consider citing Nielsen et al. (2015), Annals of Glaciology 56 (70), 70-78 as while they also find that a 2-D model is more appropriate they also compared a 1-D approach

Line 204: I think that MacGregor et al. 2009 applied a flowband model, not only the LLA so perhaps clarify why this is cited here; can see that Waddington et al. 2007 is cited in reference to the LLA itself (though they also applied a flowband model)

Line 212: "For this reason, the Nye model is generally only appropriate for IRHs found in the upper part of the ice column, as is the case here" – part of the value of using something like the LLA is that it can let you know where you may be surprised by relatively shallow layers (in the upper part of the ice column) but where the Nye model is not appropriate. This point made opens to this, but perhaps could be a chance to reiterate that it may need to be evaluated?

Line 239: Is it necessary to clarify / explain that previous study cited (Medley et al., 2014) was inferring accumulation rates on different timescales and using firn layers – right?

Line 298: Can anything more be shared about why 100 km was picked?

Line 312: Are these numbers the same as ones given in lines 299? I got confused by how these two points were stated

Line 315: Perhaps indicate that point based are "cores", since that terminology comes up later. I had to read this sentence twice so perhaps stating even more explicitly (though it does make sense)

Line 338: Maybe use another phrase than "As a result…"

Has RACMO2 modern or RACMO2 estimates for past decade(s) been evaluated against any ice-core data?

Around Line 386: Is it worth commenting on what this means for recent signal of divide migration (Conway and Rasmussen, 2008)

Line 399: The point "…a finding that must be considered by future modeling studies that simulate past sea-level rise from Antarctica since the LGM" could be strengthened to more directly speak to the modeling community (see above point)

Around Line 415: The interior thickness response to an increase in accumulation in Koutnik et al. (2016) is also tied to assumptions about how fast the limited portion of the ice sheet interior in the model responds to changes in accumulation. This was incorporated in the model based on a physical assumption of ice-sheet adjustment, but it is still an assumption (could see Koutnik and Waddington, 2012). So, while the point made here is reasonable overall, I would suggest emphasizing that tens of meters adjustment depends on the model and also depends on the initial state – the adjustment through mid-Holocene may be sensitive to initial state (early Holocene) and accumulation history for previous few thousand years. And, good that you point out that dynamic component could be important and hasn't been directly taken into account.

So, the point made that "This potential increase in surface elevation is unlikely to affect the steady-state assumption of the 1-D model used here…" isn't necessarily founded on a suite of time-dependent flowband model runs that really tell you whether 10s of meters is the right range. One run gave this range, and bigger elevation change isn't necessarily expected, but this one model run doesn't really address the range of unknowns. Just a comment! Also, may want to clarify that you mean steady-state geometry.

Line 418: I would suggest making a separate paragraph that is directed at the modeling community. This point is a good one, but stating more specifically how this could/should go would be helpful (I think)

Continent-scale modeling from LGM to present does estimate elevation change since the last glacial maximum but there are big discrepancies between models, and this problem isn't reconciled yet (though Jessica Badgeley who recently received her PhD at University of Washington is investigating this). All to say that I think not only mid-Holocene to present elevation change is important, but the potentially bigger changes considered from LGM to Holocene need to be reconciled between models and evaluated with available data (ice-core records, radar data). Is this important to address here – why the emphasis on capturing the mid-Holocene in models compared to the transition from LGM to Holocene (I think ice-sheet adjustment times work as an explanation, but could consider if you need to address that).

Lines 423-433: Is there more of a takeaway related to this work that should be made in relation to these points?

Lines 434-457: This is interesting overall, but wonder if the framing in relation to this new work should be mentioned more up front in the paragraph. It was hard to put into context where this was going until about mid-paragraph. And, the rationale for connecting increased accumulation to interior thickening to GL advance seemed

a bit tenuous given what was shared previously about not really knowing if there was thickening or thinning. The way I read this, there was a bit of back and forth in the discussion points between what may or may not be interpretable.

Would it be more impactful to consider structuring the discussion a bit more around how open questions may be addressed using available constraints, results from this work, and different types of models – what is really the path forward?

Is there a way to strengthen the point in lines 456-457, "potentially indicating that this sector is more controlled by changes in ice dynamics for which even moderate changes in accumulation rate cannot compensate" – in this case why would dynamic thinning not lead to a change in divide position, which may further complicate recovering an elevation change signal? Again, tying these points back into what is known and unknown may help.

Line 472: "making it a powerful dataset for ice-sheet models" – could be more explicit if you mean the layer data or the accumulation-rate inference? I think this is referring to the inference, but is this a data set?

How this becomes a powerful new constraint could be elaborated and clarified what classes of models would be the target. I am also not sure that the closing sentence connects clearly back to what is done here. What models use a "fixed Last Glacial Maximum value" (are these climate, ice, or both) and why does 4.72 ka constraint become so important from LGM to present – in practice what is being provided to (and suggested of) the modeling community? (see points above)